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# The Effects of Increasing Financial Incentives for Firms to Promote Employment of Disabled Workers

Jean-Philippe Wuellrich<sup>\*†</sup>

January 7, 2010

## Abstract

I study the effect of an increase in financial incentives for firms to hire disabled workers in the context of an employment quota. My results suggest that this increase had a positive impact on firms' demand for disabled workers.

*JEL classification:* J15, J23, J71, J78

*Keywords:* disability, employment quota, discrimination, employment

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<sup>†</sup>Institute for Empirical Research in Economics, University of Zurich. Address: Muehlebachstrasse 86, CH-8008 Zurich, Switzerland. Telephone: +41 44 634 37 10. Fax: +41 44 634 51 55. E-mail: wuellrich@iew.uzh.ch

# 1 Introduction

Policies that promote the employment of severely disabled individuals rank high on the policy agenda in many countries. The reason for that is that the labor market prospects of severely disabled individuals is highly unfavorable and the size of this particular group is at the same time far from being of minor importance (for more details see OECD, 2003). In one-third of all OECD countries, such as Austria, Belgium, France, Germany, Italy, Korea, Poland, and Spain, policy is based on a mandatory employment quota. According to such regulations, employers are obliged to have a certain proportion of disabled individuals among their staff, ranging from 2% in Spain to 7% in Italy. In case of non-compliance, employers are usually due to pay a tax per month for each place not filled, ranging from 0.25–4% of the monthly pay-roll of firms. However, the compliance rate only amounts to 50%, ranging from 25% in Spain to 67% in France. The main issue in current political discussion concerning such employment quotas is whether the tax is simply too low to provide sufficient financial incentives for firms to hire disabled workers. Not surprisingly, there is a strong call for increasing the tax.

This paper investigates whether the unique tax increase from € 150.- to € 196.- in July 2001 in the context of the Austrian employment quota promoted the employment of disabled workers. I propose to identify the causal effect of this 30% tax increase on firms' demand for disabled workers using the interrupted time-series approach (assessing the immediate as well as the short-run impact). This approach is appealing and in the present context superior to the difference-in-difference approach since a valid control group is unavailable. One particularly attractive feature of this study is very large and comprehensive data from the Austrian Social Security Database (ASSD) and the Austrian Federal Welfare Office (FWO). The combination of these two data sets allows me to determine the number of disabled and non-disabled workers each Austrian firm employs over time.

There are only very few economic studies that evaluate the effects of employment quotas for disabled workers on their employment.<sup>1</sup> Lalive *et al.* (2009) provide recent evidence on the effect of the Austrian employment quota shortly before the tax increase in Austria. They find that one out of twenty firms employs one disabled worker more than it would without the employment

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<sup>1</sup>Most previous studies on employment of disabled workers relate to general anti-discrimination legislation. For the U.S. context see e.g. DeLeire (2000), Acemoglu and Angrist (2001), Beegle and Stock (2003), Kruse and Schur (2003), Jolls and Prescott (2004), Jolls (2004), for the U.K. context see e.g. Bell and Heitmueller (2005), and for Germany's anti-discrimination legislation see e.g. Lechner and Vazquez-Alvarez (2003) and Verick (2004).

quota.

The remainder of the article is organized as follows. Section 2 provides an overview on the institutional situation in Austria. Section 3 describes the data. Section 4 outlines the empirical strategy and section 5 presents the results and their discussion.

## 2 Background

The employment quota in Austria is the main element of the Disabled Persons Employment Act (DPEA), which constitutes the most important instrument in the Austrian legal system to enhance the labor market opportunities of severely disabled individuals. It obliges firms to hire one disabled per 25 non-disabled workers. Firms that do not comply with this obligation are subject to a tax of currently € 213.- per month and non-hired disabled worker (i.e. the tax acts as an implicit tax on hiring a non-disabled worker if a disabled worker would be required by the DPEA).<sup>2</sup> About two-third of all Austrian firms fully comply with this requirement. The tax was steadily increased from € 118.- in 1990 to € 150.- in June 2001 according to a inflation-based measure. On July 1, 2001, however, there was a unique and considerable increase in the amount of € 46.- to € 196.-. Henceforth, it was again gradually increased. This 30% tax increase amounts to roughly 1.5% of workers' average monthly salary or 0.19% of firms' average monthly pay-roll in the Austrian private sector in 2006.<sup>3</sup> The number of disabled individuals counting for the fulfillment of the quota is non-negligible. It amounted to over 91'000 in 2005 (roughly 2% of the total workforce in Austria).<sup>4</sup>

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<sup>2</sup>The DPEA also defines how the revenues collected through non-compliance taxes are to be spent. The main beneficiaries are firms (and their disabled employees) who actually offer employment to disabled workers. These subsidies, either in form of allowances or loans, support those firms which employ at least one disabled worker. In particular, they are granted for adequate workplace accommodation, wage subsidies, work assistance, occupational retraining, or professional development. Basically, this represents a reallocation of resources from firms that fail to comply with the quota rule to firms that employ at least one disabled worker in order to compensate the latter for their effort in employing disabled workers.

<sup>3</sup>Source: Statistics Austria

<sup>4</sup>The legal status of being disabled is extremely restrictive in the context of the DPEA. The disabled is approved only if a medical expert of the FWO assesses a degree of physical, mental, intellectual or sensuous disorder, which reduces work capacity by at least 50 percent.

### 3 Data

To assess the impact of the tax increase, I use register data from two different sources: (i) the Austrian Social security database (ASSD)<sup>5</sup>, which contains detailed information on individuals' employment history since 1972 together with an unambiguous firm identifier, and (ii) register data from the Austrian Federal Welfare Office (FWO), which records the disability status of all disabled individuals. Linking these two data sets allows the accurate calculation of the number of disabled and non-disabled workers each firm employs. I create a data set with monthly reference dates from January 1999 to December 2002. I further concentrate on purely private sector firms. Finally, only firms with a firm size between 5–249 are kept (note that only 0.3 percent of all firms have firm sizes of 250 and above).<sup>6</sup> This sample consists of 2,879,025 firm-month observations (104,780 firms). For the main analysis, I use only firms that are subject to the non-compliance tax (i.e. firms with 25 or more non-disabled workers). This restricted sample consists of 500,439 firm-month observations (17,017 firms).

### 4 Empirical Strategy

I adopt the interrupted time-series approach (ITSA) (see Cook and Campbell, 1979) to identify the average treatment effect of the tax increase on the number of disabled workers per firm. This approach is appealing in the present context since there is no valid control group available.<sup>7</sup> The problem with the commonly used before-after estimator is that potential outcomes may change with time, which the ITSA tries to overcome. The idea behind the ITSA is that past outcomes are

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<sup>5</sup>See Zweimüller *et al.* (2009) for a detailed description of the ASSD data.

<sup>6</sup>For firms with less than 5 employees, employees based firm characteristics are not well defined and are therefore excluded from the sample.

<sup>7</sup>For example, a difference-in-difference approach requires a valid control group. The two obvious candidates for serving as a control group are firms not covered by the DPEA and non-disabled workers. However, both control groups cannot be used. The former control group is not applicable since the conventional DID estimator requires that in absence of the treatment, the average outcomes for treated and controls would have followed parallel paths over time. This is implausible in our context as non-treated firms are not obliged to hire any disabled workers whereas treated firms are, independently of the tax increase. In addition, this issue cannot be overcome by the recent semi-parametric approach suggested by Abadie (2005) due to the common support problem regarding the firm size. The latter control group (non-disabled workers) is not applicable since, in the context of the DPEA, firm size is defined as the number of non-disabled workers. Yet, this is an important firm characteristic by itself that confounds the number of disabled workers per firm (i.e. there is a strong positive relationship between non-disabled and disabled workers per firm) and at the same time determines whether a firm is subject to non-compliance taxation. Thus, non-disabled workers cannot be used as control group in this context either.

the best predictor of future outcomes if the policy change had not taken place. This prediction allows to calculate the counterfactual number of disabled workers at the date of the tax increase. The crucial identifying assumptions are that (i) the outcome variable is continuous in time in the absence of the tax increase, and (ii) the treatment status is not related to the policy change. The first assumption is plausible since no further policy changes referring solely to firms with 25 and more non-disabled workers took place in July 2001, which may have affected the outcome variable. The second assumption needs a closer look. Firms are free to choose their firm size and hence their treatment status. The identifying assumption fails to hold if firms that would have chosen to employ at least 25 non-disabled workers without tax increase, did not hire as many workers due to the higher tax in place. In that case, the average treatment effect is upward biased. Whether or not this assumption holds is fundamentally untestable. However, I argue that it is only sensible for firms, which are close to the threshold of 25 non-disabled workers. It is rather unlikely that firms considerably adjust their firm size in order to avoid being subject to the tax. Moreover, in the presence of any seasonality the ITSA is sensible to the distribution of calendar month before and after the policy change. Therefore, I adjust the number of disabled workers per firm for seasonality.<sup>8</sup> Moreover, I allow the time trend to differ before and after the tax increase. The idea behind this is as follows. The interruption in the time-series captures the immediate response of firms to the increased financial incentives to hire disabled workers. However, the overall effect may be bigger than this interruption actually reveals if some firms sluggishly respond to the tax increase (e.g. firms need time to search and/or to provide workplace accommodation to fill a job vacancy with a disabled worker, firms may only be able to learn over time whether they have only temporarily or permanently crossed the quota threshold, or excess employment may also build up over time due to retention). The short-run effect of the tax increase is captured by the difference in the time trend before and after the tax increase.

The following linear regression allows to identify the average treatment effect of the tax increase on the number of disabled workers per firm:

$$\tilde{Y}_i = \beta_0 + \beta_1 \cdot \text{After}_i + \beta_2 \cdot t_i + \beta_3 \cdot \text{After}_i \cdot t_i + FSD'_i \cdot \theta + X'_i \cdot \alpha + \epsilon_i,$$

where  $\tilde{Y}_i$  denotes the number of disabled workers adjusted by seasonality.  $\text{After}_i$  indicates

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<sup>8</sup>I do this by regressing the number of disabled workers per firm on the full set of calendar month dummies and use these results to predict the number of disabled workers per firms given seasonality.

whether a firm is observed before or after the policy change.  $t_i \in \{-30, -29, \dots, 0, 1, \dots, 17\}$  captures the time trend (in the baseline specification I assume a linear time trend, but it is extended by quadratic terms for robustness checks).  $FSD'_i$  is a vector of firm size dummies and  $X'_i$  includes control variables such as firm age, firm location, firm's industry affiliation, the number of non-disabled apprentices, characteristics of firm's average non-disabled workers (age, share of white-collar workers, share of women), and the median log daily wage paid to its non-disabled employees. The parameters of main interest are  $\beta_1$ , which measures the immediate response of the tax increase on the number of disabled workers, and  $\beta_3$ , which measures the short-run response to the tax increase, i.e. the change in the time trend after the tax increase.

## 5 Results and Discussion

I begin with providing some descriptive evidence. Figure 1 displays firms' average number of disabled workers (adjusted for seasonality,  $FSD_i$ , and  $X_i$ ) and superimposes a linear time trend (dashed lines) before and after the tax increase (the dashed vertical line denotes the date of the tax increase). We see that there is an interruption in the time trend at the date of the tax increase. Moreover, we see that whereas the slope of the time trend is literally zero before the tax increase, it has an strong upward trend after the tax increase. These two features provide strong evidence that the 30%-increase in the non-compliance tax had an immediate as well as short-run impact on firms' demand for disabled workers.

Table 1 shows the econometric results.<sup>9</sup> First, I discuss the immediate response to the tax

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<sup>9</sup>In order to assess the validity of the empirical design and the robustness of the results, I performed two additional analyses. First, I run several placebo regressions. The procedure is as follows. I restrict the sample to the time before the tax increase (January 1999 – June 2001), a sample that contains 311,480 firm-month observations. Then I run nine placebo regressions (linear time trend) using pseudo dates for the tax increase, which are in the center of this time-frame (December 1999 – August 2000). This choice ensures that I have a sound number of firm-month observations on either side of these pseudo dates. The results are as follows. Only one coefficient is statistically different from zero, but with a magnitude that is a third smaller than the estimate in column (1) of Table 1 (0.01324 vs. 0.02017) and with a p-value 5 times as large (0.034 vs. 0.007). None of the remaining 8 coefficients is statistically significant at the 5%-level (though four of them are marginally significant at the 10%-level) and all of them are considerably smaller than the estimate at the true date. The fact that the interruption in the time-series for the true date is larger than for any of the placebo dates (December 1999 – August 2000) and that its p-value is by far the smallest supports the plausibility of the empirical design. Second, I also checked whether the results are sensitive to the choice of the dependent variable. Instead of using the number of disabled workers per firm, I could also have defined the dependent variable as the percentage of disabled relative to non-disabled workers in each firm. The results do neither qualitatively nor quantitatively change much. This sensitivity check suggests that it is unlikely that the results are driven by a misspecification in the relationship between the dependent variable and the time trend since there is no obvious reason why the results should be similar irrespectively of the choice of the dependent variable using the same specification.

increase. Column (1) displays the result for the linear fit in  $t_i$ . The immediate response amounts to 0.0202 and is statistically significant at the 1%-level. This means that firms employ 0.0202 disabled workers more than they would in the absence of the tax increase, which is in terms of the average number of disabled workers a 1.9% increase. Put differently, roughly one in 50 firms employs one disabled worker more due to tax increase. Adding quadratic terms in  $t_i$  does not change the results much. It only makes the effect with 0.0323 more pronounced (column (2)). Column (3) performs the same analysis as column (1), but keeps only firms with at least 31 non-disabled workers (see discussion on endogeneity of firm size in section 4). It turns out that the effect does not alter as compared to column (1). Controlling for firm fixed effects in columns (5)–(7) does not change the results with respect to the immediate responses. With the linear specification of the time trend (column (5)), it amounts to 0.0255 and increases to 0.0367 when adding quadratic terms in  $t_i$  (column (6)). Column (7) shows that the result is again very robust to the exclusion of the firms with 25–30 non-disabled workers.

Second, I look at the short-run responses of firms to the tax increase. It turns out that the time trend significantly changes after the tax had been increased (expect for column (1)). The slope of the linear time trend increases by 0.0023 (column (5)). This means that roughly one in about 450 firms decide to employ one disabled worker more each month as a response to the tax increase, i.e. they indeed sluggishly respond to the tax increase. In column (6) – with a quadratic time trend – the slope in the time trend increases by 0.0035 in the first month and exhibits no statistically different decrease over time. Again, the results are robust to the exclusion of firms with 25–30 non-disabled workers. Columns (5)–(7) provide strong evidence that the time trend changes after the tax increase. Columns (1)–(3) (without firm fixed-effects) show similar results.

It is of clear policy importance (as an anonymous referee suggested) whether the positive effect of the tax increase for firms being subject to non-compliance taxation is offset by a decrease in the number of disabled workers in firms not covered by the DPEA (e.g. if disabled workers are simply lured away from small firms instead of being hired from non-employment). I investigate this issue in columns (4) and (8), in which also firms with firm size  $\in [5, 24]$  are included. In these specifications I interact the immediate as well as the short-run effect with an indicator function for firms not covered by the DPEA ( $= I(\text{firm size} < 25)$ ). The results are as follows. The immediate response for firms being subject to the non-compliance tax is 0.01837 in column (4) (without firm fixed effects) and 0.02902 in column (8) (with firm fixed effects), in each



instance a value that is very close to the magnitude found in columns (1) and (5) respectively. The interaction term between the immediate response and an indicator function for firms not being subject to non-compliance taxation are of the same absolute magnitude as the effect for firms subject to non-compliance taxation, but with opposite sign. This suggests that there is no negative immediate impact for firms with less than 25 non-disabled workers that offsets the positive impact for firms with 25 or more employees. The exact same pattern is found for the short-run impact. This strongly supports the view that the tax increase has indeed a positive overall impact on the number of employed disabled workers.

To sum up and taking column (5) (linear time trend and firm fixed effects) as my preferred specification, I provide strong evidence that the tax increase led to a immediate as well as short-run response of firms covered by the DPEA. This impact is not offset by firms not covered by the DPEA. The immediate response of firms amounts to 0.0255, meaning that one in 40 firms employ one disabled more than they would without the tax increase (in terms of the average number of disabled workers, this is a 2.5% increase). After 18 month, and taking the short-run response into account, the effect amounts to 0.0669 ( $= 0.0255 + 18 \cdot 0.0023$ ). Thus, by the end of 2002, one in 15 firms employs one disabled more due to the tax increase (in terms of the average number of disabled workers, this is a 6.4% increase). This suggests that firms' elasticity of substitution between disabled and non-disabled workers equals 2.67 ( $= 6.4/2.4$ ).<sup>10</sup> This high substitutability is not surprising in the context of the DPEA. Recall that firms can recover the costs associated with the employment of a disabled worker (see section 2) and thus the productivity gap between disabled and non-disabled workers should not differ much. I conclude that the tax increase considerably increased firms' demand for disabled workers and thus policy makers aiming at boosting employment of disabled workers should favor a further rise in the non-compliance tax.

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<sup>10</sup>The average monthly wage firms pay to their workers is € 1953. Accordingly, in terms of the average monthly wage, the increase in the non-compliance tax by € 46 amounts to 2.4%.

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Figure 1: The number of disabled workers (adjusted for seasonality,  $FDI_i$ , and  $X_i$ ) over time

Table 1: The effect of the tax increase on the number of disabled workers per firm

	Number of disabled workers (adjusted for seasonality)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean	1.03727	1.03727	1.18371	0.25974	1.03727	1.03727	1.18371	0.25974
Standard deviation	1.89322	1.89322	2.02704	0.93024	1.89322	1.89322	2.02704	0.93024
After	0.02017*** (0.00749)	0.03234*** (0.00699)	0.01994** (0.00893)	0.01837** (0.00807)	0.02552*** (0.00649)	0.03668*** (0.00548)	0.02609*** (0.00764)	0.02902*** (0.00682)
$t$	0.00001 (0.00048)	-0.00241* (0.00124)	-0.00020 (0.00057)	0.00017* (0.00054)	0.00150*** (0.00117)	-0.00064 (0.00064)	0.00155** (0.00064)	-0.00008 (0.00011)
After $\cdot t$	0.00156 (0.00098)	0.00425* (0.00221)	0.00212* (0.00116)	0.00387*** (0.00010)	0.00225*** (0.00078)	0.00347* (0.00177)	0.00257*** (0.00091)	0.00395*** (0.00062)
$t^2$		-0.00008** (0.00004)				-0.00006* (0.00003)		
After $\cdot t^2$		0.00006 (0.00010)				0.00010 (0.00008)		
After $\cdot I(\text{firm size} < 25)$				-0.01740** (0.00884)				-0.02935*** (0.007385)
After $\cdot t \cdot I(\text{firm size} < 25)$				-0.00393*** (0.00079)				-0.00395*** (0.00061)
Firm fixed effects	No	No	No	No	Yes	Yes	Yes	Yes
Firm size $\in (25, 30)$ excluded	No	No	Yes	No	No	No	Yes	No
Firm size $\in (5, 24)$ included	No	No	No	Yes	No	No	No	Yes
Number of Obs.	500,439	500,439	407,587	2,879,025	500,439	500,439	407,587	2,879,025
R <sup>2</sup>	0.28891	0.28891	0.28763	0.32917	0.03564	0.03566	0.03707	0.03701
Adjusted R <sup>2</sup>	0.28856	0.28856	0.28721	0.32911	0.03519	0.03520	0.03653	0.03692

Notes: (a) \*\*\*, \*\*, \* denotes significance at the 1%, 5%, and 10% level respectively. (b) Standard errors clustered by firm number. (c) All regressions include the full set of controls ( $X_i$ ) and firm size dummies ( $FSD_i$ ). (d) Sample: Private sector firms with 25-249 employees (columns (1)-(2) and (5)-(6)), with 31-249 employees (columns (3) and (7)), and with 5-249 employees (columns (4) and (8)). (e) Source: Own Calculations, based on ASSD and FWO